

Interactive  
Comment

## ***Interactive comment on “Concentrations and fluxes of biogenic volatile organic compounds above a Mediterranean macchia ecosystem in Western Italy” by B. Davison et al.***

### **Anonymous Referee #1**

Received and published: 19 April 2009

Davison et al, BGD 2009

First, let me apologize for commenting on this manuscript so late. It was not my intention.

This manuscript is a useful addition to the collection of manuscripts on BVOC emissions. However, because the authors make little attempt to interpret any of their data to much detail, I am afraid, that is about it. Reading the text and several similar one before that made me think about a more general issue. So let me ask what you may call a philosophical question:

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Should editors allow manuscripts to be published that are written in a "been-there-done-that"-style and therefore do not advance the state of the science significantly? Do I notice a tendency to refrain from making potentially controversial statements in lieu of papers that can be characterized as "boring"? Are we content with findings that satisfy limited expectations and the minimum publishable unit at the same time?

I have no squabble with most of the contents presented in the manuscript: - the measurement methodology is not new, it has been discussed at length before and can be considered sound under most circumstances

- the issues with the technique, such as limited count rates for minor abundance VOCs, advantage of using direct calibrations, uncertainties related to the lag-time in the tubing, discrepancies in acetaldehyde, are known

- the determined fluxes of all VOCs showed the expected behavior; if they did not, THAT would be have been new

- the data quality assurance measures seem to be standard, including both measurements and modeling in the manuscript

So what is my problem? Well, several things come to my mind. They revolve around topics that we commonly ask our graduate students BEFORE they submit manuscripts:

1. lack of why exactly this study was conducted: what were the goals? objectives?
  2. if there were objectives, were they met? if an objective was to improve modeling of isoprene emissions from Mediterranean vegetation, as suggested by text in the introduction, then the results suggest the answer is "no"; if a more general objective was to model other BVOC emissions the answer is "was not addressed".
  3. lack of interpretation: assuming that the measurements are correct, no attempt was made to interpret them beyond documentation.
- What drives methanol or acetone emissions in this ecosystem? temperature? light?

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stomatal conductance? soil emissions? plant developmental status? it is not enough to cite the literature in which these questions were addressed; YOU need to address them!

- why are the modeled BVOC emissions so far off the measurements? issues with the model setup? issues with onsite LAI or isoprene/monoterpene emitter density? issues with the measurements?

- why are all fluxes negative on the night of 9-10 May? A fluke? Would have all night-time fluxes been negative if the stability or friction velocity criteria are relaxed? if deposition fluxes are considered real, why did you not discuss them? The statement "Note however, that night time flux measurements are often associated with large degrees of uncertainty due to the stable atmospheric conditions and low wind speeds that often occur at night." is misleading, because you said earlier that fluxes made under stable conditions were discarded (why was this done anyway?). Also, your text suggests that "uncertainty" means incorrect. However, your data shows a clear auto-correlation, hence at least a qualitative correctness. Got something new here you may want to comment on?

- why do you discard BVOC flux measurements when there is no clear maximum in the covariance function between vertical wind speed and humidity? That just means there is no measurable water vapor flux; there may still have been a BVOC flux; and even if the BVOC-w covariance function does not show a maximum, that just means there is no measurable flux, not necessarily that the flux calculation method is inappropriate. It seems very unlikely that the pump acquiring the sample would have had such large fluctuations that it renders the lag time estimate so erroneous as to make it necessary to discard the data. All non-exact lag time estimates simply lead to an underestimate of the real flux (refer e.g. to B. Baker et al., 1999).

- if you discard data for the said reasons, that principally results in a "minimum detectable/acceptable flux" in your setup; what is the value of that?

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- what is "new" in your data? the discussion of BVOC abundances and fluxes does not present new insights. The following sentence is symptomatic: "Whether the elevated concentration levels observed over the first few days of the campaign were due to the heavy rain encountered during 15 this period or disturbance of the site during setting up is unclear but there are also notably higher concentrations during the early part of the second period of the campaign (20 May to 3 June 2007), suggesting these elevated concentrations may be related to the inevitable vegetation disturbance during setting up." Sorry, but I do not buy into this simplicity: (i) You did not evaluate the evidence, meaning relationships to temperature, soil moisture, stomatal conductance, NEE, etc., (ii) you did not evaluate the weather conditions, particularly as to the influence of synoptics on background VOC level, (iii) you did not name or evaluate the potential stressors, aka explain what "disturbance" is meant, (iv) your own flux data does not support the disturbance speculation, and (v) it is very unlikely that the small scale effects of a local instrument setup could affect the much larger concentration footprint, or even the flux footprint.

In lieu of going into detail on certain sections of the manuscript, I suggest to the editor to return and ask for major revisions

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Interactive comment on Biogeosciences Discuss., 6, 2183, 2009.

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